Chen, Philip S. 2001 C

Dr. Philip S. Chen Oral History 2001 C

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This is the third interview in a series on the career of Dr. Philip Chen. It was conducted on March 1, 2001, in his office on the first floor of Building 1, National Institutes of Health, Bethesda, Maryland. The Interviewers are Drs. Victoria Harden and Buhm Soon Park.

Harden: ...my memory was that you were probably Hans's closest advisor, but there were a couple of other people who were very senior on the staff, and when he came to NIGMS, of course it was after the debacle in New Jersey, and I think he was beginning to lose his eyesight already even. I mean, it wasn't bad because he obviously stayed on and moved over here. But I wanted you to make an assessment of his period as director, what the institute accomplished, you thought, what his strengths were, his weaknesses.

Chen: He, I guess, first noticed the fact that his eyesight was going bad when he was at Rutgers, and he noticed that, looking at a windowpane, the vertical things in the middle of the window had a little bump on them, and the bumps would move. So he went to the ophthalmologist and was told what this might mean, macular degeneration. But at the time I first met him in probably early '72 or late '71, he was still driving a car because he invited me to lunch to talk about moving to NIGMS.

In NIGMS, he had a close advisor named Vincent Price, Vince Price, and then Leo Von Ueller [sp.] was head of, I think, the medical scientist training program. He worked with Vince Price in that program, and Leo eventually became deputy of that institute.

I first came to his attention probably in '71 or so, maybe '70, because I was the project officer on a study of training that the Office of Program Planning and Evaluation, under Tom Kennedy, was sponsoring. The training programs were under some threat at that time, and so we decided to have a survey done of training from the standpoint of the deans, the department chairmen, professors, and the trainees themselves, and a contract was awarded to the Bureau of Social Science Research, BSSR, to conduct this survey. It was a very long, extensive effort, and I was the NIH project officer. And so during that period, I gave talks about the study to different groups at the NIH, and one of them was to the NIGMS group, since NIGMS supported a lot of training that was done by the NIH. And I guess during that period, I became known to some of the staff at NIGMS, and I had known Vince Price earlier. So when Bob Greenfield decided to retire and they were looking for a replacement, Dr. Stetten approached me since I was in the Office of Program Planning and Evaluation here at the NIH, and I decided to join him in early 1972.

During the two years that I was with NIGMS--and I had an office in Building 31, shared an office suite with Jim Dixon [sp.], the program director for the Biomedical Engineering Program--we were down the hall from Dr. Stetten, who was at the end with his executive officer, Gordon Clovedall [sp.], and so there were probably half a dozen of us, only, in NIGMS offices on the fourth floor of Building 31, A wing. The rest of the institute was in the Westwood Building, so every week we would drive to the Westwood Building to go to the meeting of the so-called scientific directorate of the NIGMS, which included all the program directors and so on there.

During this two-year period, I actually didn't get to work terribly closely with Dr. Stetten. I mean, we obviously interacted and attended meetings together, but most of my assignments passed through the executive office of Gordon Clovedall [sp.], so Gordon Clovedall [sp.] would come down the hall and he'd say, "Say, the boss wants this done," and he'd say, work on this budget thing or this congressional testimony, whatever. So it wasn't until I came over here with him to Building 1 that I got to know him very well. So it was Vince Price, I think, who actually recommended to Dr. Stetten that he bring me from the NIGMS to the Office of the Director rather than someone else. He was, of course, quite dependent on Leo Von Ueller [sp.], but Leo was felt to be more appropriate as an acting director before a new director was finally selected, and that eventually became Ruth Kirschstein. So he only took one person with him, which was myself, and we came over here in March or April of 1974.

Harden: But you didn't finish your sentence. Why were you recommended to come?

Chen: Oh, I think that's because Vince Price felt that I was the best type of individual that would be able to do the things that Dr. Stetten needed done, I guess.

Harden: And what was Vince himself going to do?

Chen: Vince stayed back in the NIGMS, and he continued on for a number of years until he finally retired.

Harden: But he didn't have any aspirations to come with Dr. Stetten.

Chen: No, I don't think so, although he had been an intramural researcher for many years in the National Cancer Institute before he went into administration. No, I don't think Vince had any aspirations about coming here.

Harden: So what you're saying, then, basically is that you didn't, you were in a position to evaluate as much Dr. Stetten's decisions at NIGMS.

Chen: Oh, yeah.

Harden: _____ that?

Chen: Well, I mean, he was very interested in the medical schools, the medical education process, in supporting the infrastructure and in supporting basic science as well as some of the clinical specialties in his job as director of NIGMS. So I think his instincts were all in the right place, even though it turned out later, of course, his heart was really in the intramural program and he had been scientific director of the Arthritis and Metabolic Diseases Institute. But he had a tremendously broad understanding of all sorts of things having to do with medical education, medical research, health care. So he was one of the most broadly based persons I know.

Park: Was it the same line of direction that James Shannon pursued during his tenure, the broad scientific research and support of medical education?

Chen: Yeah. I think it was essentially the same philosophical approach. And, of course, he knew Shannon very well because Shannon had been a resident when Stetten was an intern. And so, he remembers one anecdote. I guess he described it in his book, probably, where Shannon asked him a question and Stetten gave what he thought was a very scholarly and thoughtful answer. Shannon said, "Hey, that's the most silliest thing I ever heard of in my life."

Park: And is there a term like Shannon's school of administrators?

Chen: No, not that I have ever heard of.

Harden: No, but I would... But you're saying Shannon now, and I'm thinking more the intramural directors, and the directors before Shannon were largely intramural, I mean, directing an intramural program rather than extramural. And it's this trust in the principal investigator of the intramural programs to make their own decisions, and Stetten was squarely in that _____.

Chen: Well, no. Shannon was also an intramural scientific director...

Harden: Oh, that's right, he was. I'm sorry. You're absolutely right.

Chen: ...before he became director of NIH.

Harden: But I was thinking of him in Building 1.

Chen: Yeah.

Harden: But it wasn't a Shannon school in the sense that it... But it was an NIH school because it goes all the way back. George McCoy [sp.] and his predecessors were always very much supportive of the idea of getting good people and letting them work.

Chen: Yeah. It's interesting. Shannon was a renal physiologist, and when I went to graduate school at Rochester and was working on my Ph.D., the first papers I read were Shannon's papers, because they had asked me to work on the renal clearance of calcium in the dog, and Shannon had done all the pioneering stuff on inulen [sp.] and measuring clearances of, measuring the glomerylar [sp.] filtration rate in dogs by using inulen [sp.], so I knew the Shannon name very well.

And then later, when I had applied for a commission in the Public Health Service, I think it was Shannon who told my then-professor, Harold Hodge, that they did have a place at NIH for me. But I never got to meet Shannon until I came back to NIH in the '60s, so I don't think I ever saw him when I was here in the '50s.

Harden: Okay. Well, I'm going to let you guys go on, and I'm going to go back and go see Alan Schechter [sp.] and bring him in. His eyes are better, but he needs rides, so _____ talk.

Park: Thank you very much.

Harden: Thank you for doing this for us, and you're in good hands.

Chen: Give my regards to Bob.

Harden: Will do. And I look forward to reading a transcript of this when you get done.

Park: So, let's start with 1974, when Dr. Stetten took the position in Building 31 and, really, in one year, in 1975, there was a big gathering of former NIH scientists along with the current scientists. I was wondering, what's the purpose of that meeting? What's the motivation behind that meeting, and what's the circumstances in 1975? And who initiated the meeting?

Chen: I think it was the brainchild of Sidney Udenfrend [sp.], who had been a lab chief at one time in the Heart Institute, the National Heart Institute. In fact, his group worked right across the hall from me on the 7B corridor of Building 10. Udenfrend [sp.] I think had been a technician to Jim Shannon many, many years prior in New York City, at Goldwater Memorial Hospital. And somehow, he had gone, he had left NIH and become head of the Roche Institute for Molecular Biology in New Jersey, taking many people with him, and he was director of that institute at the time, in 1975. So I think he was the one, as I recall, whose brainchild it was to have the 1975 alumni reunion. And the keynote speaker that was invited was Arthur Kornburg [sp.], the Nobel Prize-winning person who worked on DNA polymerase. So I'm not sure who all at the NIH were involved in the planning of this. There should be documentation somewhere of, you know, we have some of the genealogies like the ones I gave you that were prepared for that.

Park: Right. But was it, did it have any purpose to send a signal to the political world that we, NIH scientists, need to get continued support of basic science and the kind of broad support of the research rather than program _____? Was there any message that was sent from the alumni?

Chen: Yeah. I'm sure that there was. And hopefully you'll be able to find some of this in some documents we have around here. I'm not sure whether, in the OD files or in my files somewhere, we can find some of this, or maybe in some articles that were written about that time. Have you been able to see any articles?

Park: I think I came across an article in *Science*, a very short article, but Arthur Kornberg [sp.] and other _____.

Chen: Oh. How about the NIH records, the old NIH records?

Park: Probably, yes.

Chen: Yeah. That would be a good thing to check. I think that's maybe the first place I would check, and I have copies of the old NIH records here you could look through.

Park: There is the _____.

Chen: Yeah. I think the NIH record would be really the best place to start to get this. And then, from the NIH records, maybe you could find some other documents.

Park: Right, right.

Chen: It's been 26 years ago, so...

Park: And do you recall any response from the NIH director at that time? The director was Robert Stone or Frederickson [sp.]? Do you remember?

Chen: Stone had probably left. It was probably Don Frederickson [sp.] at that time. Yeah. And, of course, he's still around. I bet you you could get, maybe try to get an interview with him, you know. He lives very close to NIH.

Park: I was told that he is writing his memoir.

Chen: No. He's just completed a book, which should be coming out very soon, on the history of recombinant DNA guidelines and the role that he and the NIH played in that, so it's quite a limited type of coverage. It's not a history of NIH by any means. The American Society of Microbiology is publishing it.

Park: Oh. So it's coming out soon.

Chen: It should be. He said a few months ago it was coming out in a few months, so it should be out soon.

Park: So in 1975, there was a change of directorship from Dr. Stone to Dr. Frederickson [sp.]. And do you remember any of the circumstances around that time? Was that the same kind of Marston [sp.] cut by Nixon?

Chen: No. I think that, for some reason, the people in the assistant secretary for health's office felt that Dr. Stone was, you know, they'd best replace him, and there was a man downtown called Theodore Cooper, Ted Cooper, who had been at one time the director of the artificial heart program at the NIH and later the director of the National Heart Institute. Cooper had been a professor of surgery at the University of New Mexico and I think had been under Dr. Stone, who was the dean. But apparently the two of them didn't see eye to eye, and Cooper was in a position from his office downtown, as deputy assistant secretary for health, to influence decisions which led to the removal of Dr. Stone. So I think it didn't come from the very top, like the President in the case of Dr. Marston [sp.], but it came from the level of the assistant secretary for health.

Park: And according to Dr. Stetten's memoir, he had a pretty good--he got along very well with Dr. Stone, unlike the previous expectation, because Dr. Stone sort of gave the full command of the intramural program to Dr. Stetten while the clinical program was given to the clinical director, I mean the extramural program given to the extramural director. So it was a surprise. It might be a surprise to many of the NIH scientists.

Chen: What was a surprise?

Park: You know, the resignation of Dr. Stone. How did NIH scientists receive that change?

Chen: Oh, I don't think it made much difference because most NIH scientists had very little to do with the NIH director. They're working in their labs, they're marching orders or resources come from the scientific director of the institute. So I think things that changed much higher up, like the director of NIH or the surgeon general or the assistant secretary for health or the secretary of HHS, don't have a great deal of effect on their everyday lives.

Park: I see. So kind of a remote...

Chen: It's a remote thing. Yeah.

Park: And let me go into the interesting report by Stetten in 1976. Probably you remember that--I got a copy of this from _____ NIH. In the forward, he mentioned your name as a contributor, and certainly the NIH director asked him to write about the intramural program. And why did the director ask that kind of question? Is that kind of a usual thing when the new director came in and they needed a report from the _____ director?

Chen: Yeah. Oftentimes, new directors, when they come in, will ask for this kind of a study, or sometimes it's asked during the course of one's tenure. It's not uncommon to have reports on various subjects like this. Sometimes they're initiated by a request from higher up, and sometimes the person himself or herself will initiate a report, because there are similar reports that cover the intramural NIH from Dr. Stetten's successor, Dr. Goldberger. He wrote a report, and then Dr. Rahl [sp.] wrote a report. So each of the succeeding deputy directors for intramural research or deputy director of science have written reports of varying scope on the intramural program.

Park: But this report is very unique, I think, because he wants to write it himself rather than having a committee _____, and there seems to be some kind of an interruption at some point. I don't know. Do you remember anything else?

Chen: Is that your handwriting?

Park: Yeah, it's my writing.

Chen: Okay. I don't know. Maybe, it might have had to do with his having to chair the Recombinant DNA Advisory Committee. I don't know what he's referring to by interruptions. I think it just means that it took him a while to do it, that it stretched out over a period of time, during which he' s doing other things.

Park: And in the beginning, Chapter 1, he mentioned the reasons for the intramural program. Why do we need intramural program if the intramural research is almost the same as the research done at the universities? And his answer is, "because it is there," you know, because it is there and it is excellent, and we'd better keep it as it is now. Do you think it is a strong argument or a pretty weak argument?

Chen: Well, it's one of the important arguments, but it's not the only one, and other arguments have been enunciated since. In the most recent study of the intramural program, which is the Marx-Cassell [sp.] study, they give other arguments for having an intramural program. Some of them are also in here. It's a fact that it does strengthen the overall NIH activity, you know, complements. It's not that there should be an extramural program or an intramural program, that neither one really stands up by itself, but that they mesh together, they strengthen each other. And there are certain things that are better done in an intramural program than by an extramural program, such as the long, continued studies like the Baltimore Longitudinal Study on Aging or work on slow viruses, which Carlton Gidachek [sp.] did, which probably could not have been done in an extramural, grant-supported system.

On the other hand, there are things that can be done a little bit more quickly at the NIH. When the AIDS crisis came, someone at NIH was poised, ready to identify the AIDS virus, and that was done by Robert Gallo, and the AIDS test kit came out of that work. And it was first demonstrated that AZT was effective in treating AIDS, so that AZT became the first drug approved for use in treating AIDS. These things came out, I think, more quickly because we had an intramural program with experts here and an ability to bring large members of patients for clinical trials. So the intramural program can do certain things with greater ease and greater facility than an extramural, grant-supported system.

Park: And that kind of argument was made after 1970s or in the 1980s and 1990s? Or...

Chen: Well, it's easier to make now because of things that have happened since _____. I think the same arguments might have been made back then, but there weren't as many examples to show for it. Now we can show some of these examples. It's like setting up a vaccine research center to develop an AIDS vaccine. That building was constructed very quickly, and it's up and running now. People are moving in just a few years after it was designed.

So the intramural program is able to respond more quickly to medical emergencies and medical opportunities, medical _____ opportunities, but it also has a broad set of things that are also done in the extramural program, and that's the system that you could say, it's there, it's excellent, why not continue to support some fraction of the NIH using that mode of support.

Park: I think that that kind of flexibility, with the short-term concentration and long-term investment, are very characteristic of the NIH intramural program, and the scientific directors are the persons who are in control of that flexibility, so that's why I'm very much impressed _____ intramural because scientific directors _____ this program.

But I was also intrigued by the situation that Dr. Stetten had to rationalize the existence of the intramural program in this way. I mean, that means there are somebody out there who are asking, who are questioning the reasons for the intramural program, and could you comment on that? How much pressure that the intramural directors had at the time, and what kind of pressures, if they are?

Chen: Well, I think there have always been people on the outside who would say, "Why can't the money that's going into intramural research be devoted to extramural grants so that I would have a better chance of getting a grant?" Now, these kinds of comments might not be so common when there's lots of extramural money around, but when the amount of money available becomes a small fraction of the requests for grants in that you'll hear more voices on the outside saying the money might better be spent on extramural grants, it's really a competition for resources. And I'm sure you see the same kinds of sentiments felt by people on the outside now who might be in physics or astronomy or some other field that might say, "Why is NIH getting so much money compared to us?" It's just the fact that there's not enough money to go around for everybody.

Park: And, you know, the intramural budget is only about 10 percent or not more than 20 percent of the whole budget, and the extramural program always received a higher percentage of support. And even if the intramural program support is shifted to the extramural program, that doesn't mean that outside people will receive more money.

Chen: No, that's true.

Park: But was there any jealousy about, I don't know, jealousy is the good term, but a kind of jealousy from extramural scientists about the intramural scientists, intramural scientists being not necessarily, not having to write the grant proposals and without any teaching obligations, and they have kind of free hand and they can be more productive, and it's not fair from the extramural scientists. Was there any kind of complaints from the extramural scientists in regard to the intramural scientists?

Chen: Well, I wouldn't say it's a general jealousy, but I think there might have been individual, let's say individual concerns that a certain intramural scientist who wasn't so good continued to get support.

Park: I see.

Chen: And so that relates to the fact that maybe our review system wasn't so good back then and that a lesser quality intramural scientist continued to get supported whereas a better extramural scientist didn't get a grant. I think that kind of criticism is not so common today because we have a more rigorous system for tenure, for periodic reviews by boards of scientific counselors. So I think it's the fact that maybe some extramural scientists would see, "Oh, that fellow at NIH is not a very good scientist, and yet he continues to get support."

The other possible criticism was that a few NIH scientists had big empires, you might say, large research groups that had very large budgets, and they got their names on lots of papers because they put them on all their papers. So that's, again, also been eliminated here at the NIH because we've done away with these big _____-type labs, and an individual senior investigator doesn't have the huge budgets they used to have, and so that criticism has sort of fallen on the wayside also.

So we've done away with the poor review of scientists that might have led to some of the criticisms of quality, and we've also done away with these huge laboratories with their huge budgets and huge numbers of investigators. The new model now is a smaller research group of independent researchers who are not working for a _____.

Park: I see. And how did extramural scientists convey their complaints? Did they write a letter to the director or did they say something in the review meeting, or did they write a letter to you?

Chen: No. I think it was sort of a general undercurrent that you'd hear about from time to time. And there might have been some letters written. I think I've seen some of these letters in the past that so-and-so would not get tenure at our university, or so-and-so has got too big a budget, controlling too big a budget, that kind of thing. Or there might have been some articles in *Science* or other journals that alluded to this, the fact that here was a lab chief that had lots and lots of workers and a large contract budget.

Now, many of these things were sort of cleaned up along the way. For example, it was reported that certain NIH lab chiefs had big labs that were staffed by contractor personnel, and the contractors were working, in essence, for this lab chief, so the lab chief was using contract money to aggrandize his staff. All of this was purified by removing the contractors from under the direction of... They're not supposed to be supervised by a government employee.

Park: That's very interesting because, you know, some other places, the federal laboratories like Brookhaven or in other places, the labs are big, essentially, and in the history of science, we call it big science. And here at NIH, especially when I came in 1999 and _____, I didn't see any big science going on in terms of the laboratory size. Each research _____ is pretty small size. And I don't know about the budget very much, but in terms of personnel, it's not a big size. It's a collection of small sizes and making big. So it's very interesting that there is a history behind that, making the intramural program or the labs smaller and smaller.

Chen: That's right. That's something that happened as a result of the Marx-Cassell [sp.] report. Do you have a copy of the red book that defines all of this?

Park: Do you have a copy?

Chen: Yeah. I'll get it.

Park: Okay.

Chen: Because that gives the latest report of intramural as seen by an outside committee, makes a lot of recommendations, and then it tells what the NIH did to implement the recommendations. So it's really a complete story, as of that date, of what we had done to revitalize the NIH, such as making smaller labs, making independent units rather than this big pyramidal-shaped thing with a lab chief up here and a huge budget and lots of people working for them.

Park: Was there any resistance from the intramural scientists? _____?

Chen: No, I don't think so. I think most intramural scientists have accepted this as being a good thing for them and for the NIH. It's like the more rigorous tenure system as seen in universities. It's a good thing because that increases the quality and prestige of intramural science.

Park: Do you remember some examples of big laboratories in the '70s or '60s?

Chen: Oh, yeah.

Park: Could you give me the lab name?

Chen: Well, I think some of them were like Robert Gallo, George Tordero [sp.], let's see.

Park: NCI?

Chen: Yeah. They're mainly NCI labs. I can't think of them right at the moment, but we can try to find this. They were all kind of identified at one point. _____ the size of their budgets were all identified. We'll see if we can get this. It may come to mind again. There's a couple of them there.

Park: It would be great to see how those kinds of laboratories downsized.

Chen: Yeah. Well, they were downsized by being completely modified. Most of those people have left. And, of course, along the way, they did the purification of getting the contractor people from under the direct supervision. See, some of these lab chiefs were actually in contractor buildings not on the NIH campus, and so they had their labs and offices there, and then they had contractor people all around them, so there had to be a wall set up between them, because a contract cannot be supervised by a government employee. You pay the contractor to do something and the contractor does it, but you can't order the contractor to do something. It's specified out in the contract what they do.

Park: Right, right. Let's move on to another topic that's in this report. It is about NIH, the identity of NIH. And in the last chapter, Dr. Stetten says that the intramural NIH is essentially an educational institution. And I think he has an ambition of having a degree-awarding system in it, not just about the preceptor-postdoc relationship. And was it his own idea, or is it a kind of prevalent idea among the scientific directors?

Chen: No. It was mainly his idea, because he had been the founding dean of _____ Medical School. He was a very well-respected educator. In fact, when he was himself a young faculty member at Harvard, they used to say that his lectures were the best-attended lectures. He taught some basic science to clinical people or vice versa. So he was really an educator at heart. This was his idea, which he tried out on the scientific directors back then, and they didn't buy it, so they were not enthusiastic about it.

Now, more recently, in the last few years, say the last three years, that idea was revived here. Dr. Gottesman [sp.] was actually trying to favor it. But, again, it was shot down, not so much by the scientific directors this time, but by the external advisors to Dr. Varmus, who said we shouldn't do it because there's too many graduate schools now and they all--you know, they're hurting for good students, and if you build up another graduate school in the country, _____ just take away our best students, so that was what happened. So we have not pursued the graduate-school idea, but, rather, shifted our attention to setting up graduate partnerships in existing graduate schools who would send their students here to do their thesis research. The degrees would be given by the schools themselves.

Park: Actually, Dr. Stetten and the scientific directors, in the late 1950s and early '60s, tried to get a kind of graduate program using FAES, and they tried to do that, but they didn't go, they couldn't go through, and instead Johns Hopkins University and NIH joint program started at that time.

Chen: So we're going to have lots of those joint programs now. I mean, there was one developed with George Washington University on genetics. There's one with the University of Maryland on cell biology. So now Mary DeLong [sp.] is developing these. There may be 20 or 30 or more schools that will have these agreements. Some of them are from overseas as well. This is called the GPP, Graduate Partnership Programs.

Park: When did this start?

Chen: Just starting now.

Park: Just starting now.

Chen: Yeah. I think she's in the process of going around and talking to schools.

Park: I see.

Chen: She's had one big meeting where she brought people from a number of schools here. They all talked together.

Park: How is the response from the university people?

Chen: I think it's good, yeah. There are a number of things that need to be worked out, such as, how do you pay tuition to these schools? Each school has a different tuition structure. So rather than paying the schools' catalog tuition, maybe there can be some negotiated amount that NIH would pay to each school, just like in the case of training grants, extramural training grants, such as how her office would be supported to administer these agreements. So we're thinking of charging each institute so much per head for each graduate student in these programs. The institute would pay something to her to keep her office operating. So there have to be a lot of details worked out, including how you bring foreign students, because they would need to be on a visa of some type. So she will be _____ on this, working on this program.

Park: And between 1976, when Dr. Stetten clarified his idea of a graduate program, and 1997 or so, when Dr. Gottesman [sp.] tried to refresh it, was there any--in between, was there any attempt to have that kind of graduate program, or not?

Chen: Not a formal degree program, no. And as I said...

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Chen: ...they felt that it would maybe dilute the efforts of the research program, because we would have to develop a faculty who would have to devote some of their energy to this degree-granting program instead of doing research. So I think even back then, the scientific directors were really favoring a full-time research career at the NIH.

I would say in the intervening years, there has been a gradual increase in interest by the scientific directors to where they would now favor, or certainly support, a degree-granting effort, but it's other political factors that are causing it not to come to pass.

Park: Do you know the reason why the scientific directors these days are interested in having a graduate program?

Chen: Well, I think they feel that it would be beneficial in bringing good young people here in their overall recruiting efforts. It's a good thing to have excellent young people here. And I think it will also, they feel it would attract good faculty, faculty were bringing more senior staff from the outside who have traditionally had a graduate program in their universities. So I think they would not be averse to having a formal program.

Park: Well, it's 10 o'clock now, and thank you very much for today, and see you.

Chen: Okay, very good.

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